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Biogeosciences Discussions

Interactive comment on "Phosphatase activity and organic phosphorus turnover on a high Arctic glacier" *by* M. Stibal et al.

Anonymous Referee #2

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In this paper the authors describe phosphatase activity in sediments associated with cryoconite hole debris from a glacier in Svalbard. Using a variety of incubation experiments the authors demonstrate that the microbial community associated with the cyroconite is phosphorous limited and from these incubation experiments calculate the expected turnover time for organic P on the glacier surface. I find this line of research compelling, interesting to the general scientific community and suitable for publication in Biogeosciences. Clearly the authors have a nice dataset; however I do feel the manuscript would benefit from a thorough revision to clarify the approach and results. Elements of the methodology should be further clarified; specifically regarding the P measurements made and on how the dilution (1:500) of the cryoconite sediments with buffer affects the results. In general the text could be revised to report the story in a more concise manner, often times the writing led to some confusion. Several points

of concern are detailed below: General points: Was particulate phosphorous measured? Is this not important for the major conclusion that bioavailable debris-bound organic P is sufficient for an entire ablation season? What is the average length of a Werenskioldbreen ablation season? Why is the organic P referred to as in the organic phase? The paper seems to do some hand-waving about the state of the P available. It is my understanding that SRP is now used so as not to assume that the state of the phosphorous is necessarily dissolved or inorganic as it is a functional definition. Given the extensive discussion about P, the form of P available to the cryoconite community (i.e. organic or inorganic) I think the authors should take some time to clarify specifically what their methods measure and what they can accurately infer from these measurements. Also, some clarification on what values are 'assumed' from previously published reports would be helpful.

Specific points: ABSTRACT: Line 10: this sentence is a run on INTRODUCTION: The introduction does not read so well. Statements should be clear and concise when describing cryoconite hole formation. For example, why describe wind-born debris as 'dark' without discussing the role of albedo? Line 1 page 2700: What nutrients are the authors referring to? Carbon, P, N, S? Supplying numbers or a comparison (such as to the redfield ratio) would be informative to the reader and place the cyrconite system in some context. Line 5 2700: this first sentence should be referenced The authors should describe how it was determined that the P was bound to the debris, since this becomes important later in the text. The authors only refer to studies of glacier geochemistry on Arctic systems, which is fine but this should be clarified (as there is significant research on Antarctic and temperate glaciers as well). However the authors point out that there is currently no similar data on Arctic environments (compared to the Antarctic). If the comparison between poles is to be made, the background description of the systems should be more balanced. It was a bit confusing as to whether the authors were referring to all glaciated systems or just those previously studied by the authors in the Arctic.

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Methods: Line 18: Statements like 'very low' should be qualified. So the TDP is very low relative to what? Section 2.1 reads like either background or results. What is the average length of the ablation season in days (with associated standard deviation)? This section should only include the description of the glacier and the sampling methods. The previous work should be either in background or discussion or separate from the study site description. Line 24-26: How was 'readily available P for microbes' determined? Section 2.3 line 7; how does the 50 umol of PAR compare to what the glacier surface receives? Has this been measured? This will help the reader get a sense as to how close to in situ conditions the experiments were. Is there any light attenuation by the polypropylene (just curious, with a PAR sensor that should be easy to measure)?

Line 26 on (page 2702) is a significant point, it describes the extent the samples are diluted. The associated description is a bit confusing as written, perhaps the authors can comment on how this dilution may affect microbial activity (as this seems like a significant deviation from in situ).

Line 13 pg 2704: was the SYBR Gold diluted? It comes fairly concentrated. Lines 6-8 pg 2705: are repetitive. MUP and MU have already been defined. Is it necessary to state the decline rate? Can it not just be described as following zero-order kinetics with a vmax of XX. I got confused when trying to read the results regarding additions of inorganic P under light/dark, long and short incubations. . .and was going to suggest a table, but then noticed there is a table 2 (This table was referenced in the text). Pvalues are only provided on certain occasions but should be included whenever 'significance' is invoked.

Discussion: Lines 13-17: how does wet chemical sequential extraction suggest that there is more potential bioavailable P? Was more P released following each sequential extraction? This is also a run on sentence. The paragraph that starts on line 24 seems out of order, either it belongs in the introduction/background or it should go with the paragraph where the rates found in this study are compared to Antarctic data (starting on line 23; pg 2708).

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The paragraph that starts “in this study... is out of order see above Lines 9-11-this sentence is not substantiated: are you implying that all microbial activity and cells are associated with debris [in the cryoconite]? Then this needs to be stated. There is also the Foreman et al study which looks at activity associated with cryoconite water vs. debris. Is the conversion factor of 2 (debris water) what you find in the cryoconite itself or related to the 1:500 dilution used in the phosphatase experiments? And how comparable are your results from the experimental dilution to in situ conditions? Line1-5 pgs 2709: Can some stats be done on the comparison to Antarctic data (rather than ‘mostly higher’ and ‘somewhat higher’). Higher by how much?

Line 14 pg 2709: was ‘fresh’ phosphatase production measured?

Line 23-26 pg 2708: the authors suggest that dissolution of debris may release SRP into solution. How does the dilution factor (of 1:500) affect dissolution? It is a stretch to relate faster activity at 30oC to meaning something specific about adaptation. Yes the enzyme or organism grows fastest (not necessarily optimally) at 30oC however there is still significant accumulation of MU at 0oC according to figure 4. In fact the accumulation at 0oC at 100um MUP is higher than the values of MUP-100 uM in the light at 5oC from figure 3). Seems like they would utilize more P at higher concentrations, not just higher temps ... downstream or wherever. I find the statements starting at line 5 (pg 2710) to be hand-waving.

Line 13 pg 2710- I don’t exactly follow the logic that higher phosphatase activity in the dark implies heterotrophic microbes are responsible for the majority of phosphatase activity. If heterotrophs were consistently active under light and dark conditions, activity would be equal under both conditions (if no phototrophs were active)-right? or lower when the portion of phototrophic activity ceased. Is it being suggested that these heterotrophs are somehow inhibited by the light or photosynthetic activity? Can this be substantiated? Perhaps a few statements and references to this effect should be added. Section 4.4 is the main point of the paper and should be strongly written. For example line 22 states the lower values are ‘probably more realistic’...what is the

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reasoning? tell the reader why. Earlier in the paragraph the authors argue why their estimates may over and under estimate in situ phosphatase activity, then suggest this lower value is probably more realistic without really stating why. I think the estimations of turnover times can be stated more clearly (i.e. line 23) for example: rather than "If DOP, whose concentration... Try- Is 0.2 uM (REF). We calculate that the cryoconite microbial community could turnover OP in xx hours based on our estimated value of XXX nmolpergramperL of phosphotase activity and in situ DOP concentrations of ... (for example).

Interactive comment on Biogeosciences Discuss., 6, 2697, 2009.